archipelagoes are treated with equal thoroughness, and the work is provided with a map of the Indian Ocean, an index, and numerous well-executed woodcuts.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Physiological Selection and the Origin of Species

IN the *Journal* of the Linnean Society (Zoology, No. 115, 1886, p. 350, footnote) Mr. Romanes says: "I cannot find that any previous writer has alluded to the principle which it is the object of the present paper to enunciate, and which is explained in the succeeding paragraphs."

But in the fourth edition of the "Origin of Species" (1866), p. 311, the following passage occurs, in which the main idea of "physiological selection" is clearly alluded to.

"It may be admitted, on the principle above explained, that it would profit an incipient species if it were rendered in some slight degree sterile when crossed with its parent-form or with some other variety; for thus fewer bastardised and deteriorated offspring would be produced to commingle their blood with the newly-forming variety."

The author then goes on to show that, as he believed, this kind of sterility could not be increased by natural selection—a discussion with which I am not now concerned. I have other evidence to show that my father was familiar with the principle of physiological selection, and, moreover, that he did not regard it with any great favour.

In Mr. Belt's "Naturalist in Nicaragua" (1874), a suggestion is made, identical with that of Mr. Romanes in the Linnean *Journal*. Mr. Belt says (p. 207):—"The varieties that arise can seldom be separated from the parent form and from other varieties until they vary also in the elements of reproduction. . . . As long as varieties interbreed together and with the parent form, it does not seem possible that a new species could be formed by natural selection, excepting in cases of geographical isolation. All the individuals might vary in some one direction, but they could not split up into distinct species whilst they occupied the same area and interbred without difficulty. Before a variety can become permanent, it must either be separated from the others or have acquired some disinclination or inability to interbreed with them. As long as they interbreed together, the possible divergence is kept within narrow limits, but whenever a variety is produced the individuals of which have a partiality for interbreeding, and some amount of sterility when crossed with another form, the tie that bound it to the central stock is loosened, and the foundation is laid for the formation of a new species. Further divergence would be unchecked, or only slightly checked, and the elements of reproduction having begun to vary, would probably continue to diverge from the parent form, for Darwin has shown that any organ in which a species has begun to vary is liable to further change in the same direction. Thus one of the best tests of the specific difference of two allied forms living together is their sterility when crossed, and nearly allied species separated by geographical barriers are more likely to interbreed than those inhabiting the same area."

In my copy of Belt's book the words "No, No," are pencilled in my father's handwriting on the margin, opposite the sentence "All the individuals might vary in some one direction, but they could not split up into distinct species whilst they occupied the same area and interbred without difficulty."

Cambridge, August 27 FRANCIS DARWIN

NEITHER Mr. Galton nor Mr. Meldola have had time or opportunity to consult my original paper before writing their comments on the NATURE abstract. I will, therefore, consider those of their remarks which have been anticipated in the paper.

Mr. Galton writes:—"It has long seemed to me that the primary characteristic of a variety resides in the fact that the individuals who compose it do not, as a rule, *care to mate* with those who are outside their pale, but form through their own sexual inclinations a caste by themselves." Now, I have fully recognised this principle as one among several others which is accessory to, although independent of, physiological selection: see L. S. paper, p. 377, where also reference is given to the "Origin of Species," showing that this factor was likewise recognised by Mr. Darwin as one of importance in the prevention of intercrossing. But, inasmuch as this factor—which may be called psychological selection—can only apply to the case of the Vertebrata,[1] I am disposed to think that it is of much less general importance than the other factors which I have mentioned as accessory to physiological selection, and which, taken altogether, furnish a complete theoretical explanation of the fact that sterility between natural species is not invariably absolute, but occurs in all degrees. For, "in all these cases where the principles of physiological selection have been in any degree accidentally assisted by other conditions, a correspondingly less degree of variation in the reproductive system would have been needed to differentiate the species" (p. 377).

Thus far, therefore, Mr. Galton is really in full agreement with me. But he goes on to say:—"If a variety should arise in the way supposed by Mr. Romanes, merely because its members were more or less infertile with others sprung from the same stock, we should find numerous cases in which members of the variety consorted with outsiders." But how can we possibly know that such is not the case? If my theory is true, it must follow, as Mr. Galton says, that such unions would be more or less sterile, and, as this sterility is itself the only variation which my theory supposes to have arisen *in the first instance*, *ex hypothesi* we can have no means of observing whether or not the individuals which present this variation "consort with outsiders," or with those individuals which do not present it. Lastly, in as far as it is true that "we hardly ever observe pairings between animals of different varieties when living at large in the same or contiguous districts," the fact in no way makes against my theory of physiological selection: it only serves to supplement this theory, in the case of higher animals, by what I regard with Mr. Galton as the proved facts of psychological selection.

The letter by Mr. Meldola is a masterpiece of Darwinian thinking, and on this account I am glad to find myself much more in agreement with him than he appears to suppose. For when he reads my full paper he will see that I have taken precisely the same view upon natural selection as a possible cause—or, rather, accessory promoter—of specific sterility as that to the statement of which the larger part of his letter is devoted. I may remark, however, that of all parts of my paper I regard this as the most speculative and least secure. And this, first, because Mr. Darwin himself, after profound meditation upon the subject, came to the conclusion that natural selection could not operate so as to induce sterility; and, next, because the supposition that it does so operate involves one of the most difficult and complex questions in the whole philosophy of evolution—namely, whether it is possible for natural selection to modify an entire *type* without reference to benefit of its constituent *individuals*. Now, although for reasons which need not here be detailed, I have been led, like Mr. Meldola, to take a different view from that of Mr. Darwin, and to conclude that natural selection may benefit the type without reference to the individual, still I regard this conclusion as so highly speculative that I am glad to think the much more certain theory of physiological selection is not vitally affected either by its acceptance or its rejection. If it is true that natural selection may be able to modify an organic type (as my critic and myself agree in arguing, the type in this case being a variety) by conferring on it the benefit of sterility with its parent form, notwithstanding that this cannot be effected through benefit conferred on any of the constituent individuals, then all we have to say in the present connection is that natural selection is probably one of the many other causes which lead to physiological selection.

[1] A corresponding but not identical passage occurs in the sixth edition, p. 247.

[1] This, at least, is what I state in the paper. Mr. Galton, however, suggests that the principle may be extended even to plants, through "the selective appetites of the insects which carry the pollen." This suggestion is unquestionably original, and bears the stamp of its author's ingenious mind. Moreover, considerable probability is, I think, lent to the suggestion by the observations of Mr. Bennett and others on individual insects selecting similarly coloured flowers on which to feed (see *Journ. L.S.*, 1883).

On the other hand, if natural selection cannot thus operate, all we have to say is that there still remain many other causes adequate to explain the occurrence of physiological selection—to wit, those causes which are concerned in the occurrence of variation in general.

The essay by Prof. Weismann on the influence of isolation, to which Mr. Meldola refers, is so replete with facts and arguments unconsciously bearing on my theory, that in writing my preliminary paper it appeared advisable to reserve so rich a mine for subsequent working out in detail. In my paper, therefore, I have merely alluded to Prof. Weismann as one among the comparatively few evolutionists who have hitherto sufficiently considered the influence of independent variation (or the prevention of intercrossing) in the evolution of species.

It only remains to consider Mr. Meldola's extremely able criticism of my view that natural selection ought not in strictness to be regarded as a theory of the origin of species, but rather as a theory of the development of adaptive modifications. My argument is that natural selection can only be a theory of the origin of species in so far as species differ from one another in points of utilitarian significance; and that even then it is only a theory of the origin of species, as it were, incidentally: the *raison d'être* of natural selection is in all cases that of evolving adaptations (whether these be characteristic of species only, or likewise of higher taxonomic divisions); and if in some cases the result of performing this function is that of raising a variety into a species, such a result is merely collateral, or, in a sense, accidental. No doubt if species always and only differed from one another in points of utilitarian character, the collateral nature of the result might be disregarded, and the theory would become a theory of the origin of species in virtue of its being a theory of the development of adaptations. But, as a matter of fact, species are very far from being always and only distinguished from one another in points of utilitarian character, and in so far as they are not thus distinguished natural selection is obviously in no sense a theory of the origin of species. Again, and more particularly, the one feature which more than any other serves to distinguish species from species is that of mutual sterility, and it would be a bold flight of speculation to affirm that this has been in all cases the result of natural selection, when even Mr. Darwin was reluctantly compelled to conclude that such could not be the result of natural selection in any case. On the other hand, my theory of physiological selection explains this very general feature of specific distinction quite independently of natural selection; and then goes on to show that, when once this primary distinction has arisen, many others of a secondary kind will ensue, both with and without the assistance of natural selection.

Now, the objection which Mr. Meldola adduces against this argument is that I have not proved physiological selection to be independent of natural selection. In other words, he does not dispute the probable truth of my theory; but he says that, granting its truth, it is still only "one particular phase of natural selection." But surely the burden of proof here lies on the side of my critic. If he can show any sufficient reason for going much further than I have ventured to go in out-Darwining Darwin—or for holding that natural selection may not merely help in inducing sterility in some cases, but has been the sole cause of it in all cases—then I should welcome his proof as showing that the principles of physiological selection ultimately and in all cases rest on those of natural selection. But, clearly, it is for him to prove his positive: not for me to prove what I regard as an almost preposterous negative.

So much for the main criticism. But he adds this rider, namely, that, as the struggle for existence is always most severe between the most closely related forms, unless the new or sexually protected form arising under physiological selection possesses some distinct advantage over the old or parent form, it will be exterminated by the latter quite as effectually as it would be by intercrossing in the absence of physiological selection. To this I may answer in the words of my full paper:—"So long as there is no actual *detriment* arising to the variety on account of its being sexually separated from the parent, any ideas derived from the theory of natural selection are plainly without bearing upon the subject" (p. 406). In other words, so long as in all other respects of organisation the sexually separated variation is not less "fit" than its parent stock, so long there is no reason to anticipate any disadvantage in the struggle for existence. And forasmuch as the sexual separation arises only by way of a variation locally affecting the reproductive system, when the variation is first sexually separated, it will in all other respects resemble its parent stock, and so be able to compete with it on equal terms—mere numerical inferiority being without significance where intercrossing is prevented. It was in order to convey this meaning that I proposed as an alternative name of my theory, "Segregation of the Fit"; seeing that before any physiological segregation can take place there must be organisms to be segregated, and that unless these organisms had already proved themselves fitted to survive in the struggle for existence, in existence they could not be. But I do not call physiological selection "Segregation of the *Fittest*," because, unlike natural selection, it is in no way concerned with the principle of conflict. So long as the organisms which have been separated by physiological selection are sufficiently fit to have previously passed muster at the hands of natural selection, there is no reason why the daughter type should be fitter than the parent.

But, so far as I can see, the only material point of difference between Mr. Meldola and myself consists in his regarding physiological selection as "subordinate" to natural selection, while I consider the two as quite independent principles, although, as explained in my paper, I believe that they frequently and in several ways play into each other's hands.

GEORGE J. ROMANES

Geanies, Ross-shire, N.B., August 30

Earth-Currents and Aurora

THERE appears to have been a very remarkable and widespread earth-current storm on March 30 last, full particulars of which it would be extremely useful to have on record. My attention has been drawn to this storm through witnessing, on the evening of that day, one of the most vivid and interesting displays of the aurora that I have ever seen. Mr. G. H. Kinaban, writing in NATURE for April 8 (p. 537), describes the same aurora as observed by him in Donegal between 8 and 9 p.m., and notes its peculiar bright silvery type. It must, however, have been a far less imposing display at Donegal, where the weather was less favourable, than at Kingstown, where I saw it between 9 and 10 p.m., the most brilliant display occurring between 9.30 and 9.45 p.m. From the northerly horizon there rolled up to the zenith in quick succession streamers and masses of white light, until the whole face of the sky to the north and west was illuminated with swiftly mounting flames of silvery whiteness and wonderful beauty. A correspondent in NATURE for April 15 (p. 559) describes the same aurora witnessed by him between 8 and 11 p.m. on March 30, at Königsberg, in Prussia.

According to the *Electrician* (April 2, p. 404), on the morning of March 30 a violent earth-current storm occurred in London, stopping all telegraphic work for some time. During the same day strong earth-currents are reported on the Mediterranean cables, and in the afternoon of that day (March 30) this terrestrial electric storm had reached India, lasting from 2 to 5 p.m., and stopping work on the Bombay and Madras line. At the same time powerful earth-currents are reported on the Madras and Penang cable, causing work over it to be stopped from 3 till 7 p.m. on March 30. Similar disturbances are reported on the Java cable, beginning the same afternoon, and becoming fainter on all lines at 10 p.m. Recently an account has been published of a great earth-current storm on the China and Japan cable on March 30, and a diagram of the perturbations produced on that line is given in the *Electrician* for August 6. The storm began between 4 and 5 p.m., Shanghai time, on March 30, and lasted till 11 p.m. that day, the maximum strength of the earth-current on the cable occurring between 6 and 7 p.m. being then equal to 3·6 milliamperes. Smaller renewals of the storm took place both in Europe and Asia on the morning of March 31.

Perhaps some of the readers of NATURE can give further details of this extensive storm, and it would also be useful to have on record the magnetic perturbations noticed on this occasion in different localities, and the time of their occurrence.

W. F. BARRETT

Royal College of Science, Dublin

Chlamydomyxa in the Engadine

YOUR readers will be interested to hear that I have found here in Pontresina the very interesting Protozoon described twelve years ago, in the *Quarterly Journal of Microscopical Science*, by